

ESSAYS
of an
INFORMATION SCIENTIST
by
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with a foreword by
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FOREWORD

by

Joshua Lederberg

It is a privilege and a pleasure to be able to have these pages of foreword to Gene Garfield's essays. My first encounter with Gene dates back to the publication of his landmark article in *Science*,¹ "Citation indexes for science; a new dimension in documentation through association of ideas". A few years later I recalled this first paper and asked myself whatever might have come of his proposal. Then, realizing that such a question embodied one of the principal functions of the *Science Citation Index*, I wrote to him about this private experience and offered to lend whatever public support was possible to the realization of his vision. Believing, I think correctly, that geneticists might be especially preadapted to sympathizing with the concepts of parent-offspring relationships of publications, I suggested to him that this discipline might be a favorable arena in which to test his ideas. This indeed proved to be helpful in their consummation.

I have never ceased to be amazed at the energy and insight that Gene has been able to exert to make these dreams into a tangible reality in a way that has already altered both our practice and concept of scientific communication today. There have after all been few fundamental qualitative changes in the pattern of scientific communication since the invention of the scientific journal in 1664. With the appearance of the *Science Citation Index* it is of course much easier for the authenticity of older work to be more readily evaluated. Perhaps equally important, contemporary authors realize that their tribute to

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the past history of their area—their bibliographies—will also be the key to their visibility to the scholars of the future. In this respect, Gene Garfield has helped to restore historic sensitivity to scientific description, so notoriously rare among scientific specialists.

If these remarks have dwelt so long on citation indexing, it is because this best exemplifies Gene Garfield's approach to the role of communication in science: it is no mere supporting tool or lubricant; it is as important a part of the stuff of science as is the work at the laboratory bench and with the calculator. Only that enthusiasm and self-appreciation could have lent such vitality to the innovations and services that Gene has made available to all of us; and which I have been glad to try to reciprocate in a personal way, and more recently as a member of his board of directors.

One of the virtues of the print medium by which *Current Contents* is distributed is that it offers the opportunity for the *Current Comments*, the weekly editorials that are reprinted now in these volumes. Here Gene has displayed his enthusiasm, his deep insight into the scientific process, sometimes a candor and artlessness in the expression of his feelings that may even offend some who have not also experienced his own scrupulous integrity at first hand.

The most exciting aspect of the *Science Citation Index* was the perspective that it offered on the lineage of ideas. But Gene also found that he had discovered a new tool for quantitative sociological study, a way of estimating 'intellectual influence' of one person's writings on another or on a field; or in other aggregates, the communicative structure of a field, even the differentiation of all of science into clusters of internally communicating specialties. I must confess that I was rather slow to understand why this was important at all. Perhaps it was because I had little comparative information on the rigor of other quantitative measures of social influence. Over the years we have continued to debate about the reliability of numeric measures, based on the counting of citations, for drawing inferences about the 'impact' of a particular publication or a particular author. Gene has devoted many of the pieces that are reprinted here to his own critical examination of this

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question, and the reader can enjoy for himself an appreciation of the continued growth of perspective and understanding of these problems that could only be acquired by pragmatic experience. The only thing I would have chided him for is underestimating the manner and extent to which others would be tempted to abuse these measures when it suited their own private ends.

There remains an undeniable residue of controversy about the value of publishing statistics about the distribution of citations to individual articles. This may have the merit of drawing attention to the problematics of the citation process, about which more will be said below, but certainly it can also give misleading impressions about the significance that can be attached to the numbers. I know that Oliver Lowry has been the first to protest the fact that his own paper on a method for protein analysis²—the all-time favorite among cited articles—is not at all revealing about the structure of biochemistry, or about the significance of this piece of work in either the framework of biochemistry in general, or that of Lowry's own formidable scientific contributions. The anomaly does provoke us to think a little more deeply about the circumstances that impel an author to include a familiar article in his explicit bibliography. For lack of any better theory to account for it, I have speculated myself that imitation is the self-reinforcing influence; that authors will make explicit citations to method papers, when they have seen similar explicit citations in the papers they themselves have recently read. But a speculation like this is already an assertion about communicative behavior of scientists that should not be accepted at face value and warrants critical testing in other contexts.

Citation statistics like this are even more puzzling in the light of the contrary phenomenon of 'obliteration'. The work that *everybody* knows, we quickly find, is often hardly cited at all! If anyone questions this, I suggest that he look up the entries under Jansky, the founder of radioastronomy. There are many other such cases, e.g. Beadle and Tatum 1941,³ or Avery et al. 1944.⁴

Sometimes, we will see a combination of both processes. For

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example, it was brought to my attention that some of my own work on replica plating⁵ was one of the group of 'frequently cited papers' that *Current Contents* invited authors to make some comment about. This is certainly not the item I would have chosen from my own bibliography for note-worthy impact (the ones that I would have elected have long since been obliterated). But while this has been *over-cited*, as a method paper, many articles that include *replica plating* in their titles quite comfortably obliterate any reference to Lederberg 1952. So, even in these cases where there is no inherent question about the ideological connection, we are still quite perplexed about citation behavior.

One other wrinkle showed up when there seemed to be a sudden rash of references to LeBel 1874. Was this a sign of some rediscovery of his introduction of topological concepts into organic chemistry? Well, here again was a case where the formal structure of the links between references might have been pursued at great length without reward; a glance at the actual content of these new citations showed that they were motivated by the occurrence of the *centennial* of LeBel's work.

Historians of science have sometimes also been misled by undue reliance on the formal citation network. For example, both Stent⁶ and Wyatt⁷ have argued that O. T. Avery's discovery (that genetic transformation in bacteria was accomplished by DNA)⁴ was neglected for several years after its publication in 1944. In fact, this work was so dramatic that, during the period in question, it was cited only indirectly, or by reference to later reviews, in many articles whose authors felt it was no longer necessary to quote the original publication. We have then a measure where *under-citation* can sometimes be used as evidence of extraordinary impact! In such circumstances, needless to say, one has to be extremely careful about attributions based on a mechanical application of the tool without an intimate factual understanding of the scientific and historical context of the situation.

Almost all of these remarks are anecdotal and do little justice to the scientific challenge that is posed by the data that have been accumulating and are now readily available through the *Science Citation Index* system. Here irrevocably on the record are reports of the

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actual behavior of large numbers of scientists and authors reciting their debt to traditional knowledge in a wide variety of contexts. Most of the speculations that have been offered about explanations for citation behavior, and the use of citation metrics for the evaluation of scientific significance, should be amenable to independent objective validation. This is one of the tasks for the near future, one which will also be aided by these same information retrieval tools, but will require all of the additional ingredients that can be furnished today only by human skepticism and imagination.

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